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Interactive comment on "Lateral carbon fluxes and CO_2 outgassing from a tropical peat-draining river" *by* D. Müller et al.

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Reply to Anonymous Referee 2

We thank you for your feedback and your helpful comments on our manuscript. We provide detailed answers to your comments below.

GENERAL COMMENTS Tropical peatlands are under tremendous pressure from extractive industries (e.g. timber, mining, etc.) and agriculture, with potentially wide ranging consequences for regional/global C balances, climate, water quality and biodiversity loss. We have very little data on fluvial C fluxes from many tropical peatland ecosystems, particularly from Southeast Asia, where the pace of land-use change has been extremely rapid in recent decades. This manuscript

is therefore interesting and novel because it is one of the few studies to provide baseline data on fluvial C fluxes prior to land-use change, and could serve as a useful touchstone for future studies of anthropogenic impacts on C fluxes from Southeast Asian peatlands.

However, there are some limitations to the research here; for example, the approach used to estimate annual fluvial C fluxes is based on the calculated difference between precipitation (PT) and evapotranspiration (ET) (see page 10400), and is subject to uncertainties in measured rates of ET and the assumption of steady-state conditions.

Fluvial and gaseous measurements were also only conducted in a single season in both years (i.e. post-monsoon). Lastly, gas evasion measurements provide only a partial picture of water-air exchange, because there are questions as to how spatially representative the measurements were, and if water-air exchange is influenced by wind/turbulent flow (although the authors argue this is a non-issue because of relatively sheltered river conditions).

Yet despite these limitations, I believe this study makes a valuable contribution to the wider literature on tropical peatlands, because so little is known about undisturbed peatland systems, particularly in Southeast Asia. Moreover, much to the authors' credit, they have openly and transparently discussed the potential sources of uncertainty in their measurements (see section 4.4 in the Discussion). This is to be commended because it enables readers to assess the data for themselves, acknowledges any potential biases in the estimates of C flux, and also provides a starting point for identifying how future studies of this kind could be improved. In my overall assessment, this is good work, given the challenging field conditions and limited infrastructure, and will extend our knowledge of these regionally/globally important but understudied ecosystems.

Specific comments on individual sections of the text are provided in the section

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below.

SPECIFIC COMMENTS

1. Page 10395, line 19: Use of abbreviations like "NP". This is a subjective stylistic point, but where possible, I think the text would read more elegantly if abbreviations were only used sparingly. While in some commonly-used terms like "Peat Swamp Forest" (PSF) might be better abbreviated due to their length, other shorter terms (like "National Park") may be better referred to in full. Abbreviations tend to interrupt the flow of the text, and I prefer only using abbreviations for very wordy or long terms.

We avoided the abbreviation "NP" in the revised manuscript.

2. Page 10396, line 27 to Page 10397, line 29: In the section on sampling procedure and instrumental analysis, it would be useful for the authors to state the precision of their measurements for the TOC, AMS and IRMS (typically reported as the Coefficient of Variation for the standards). Precision is reported for the IRGA/headspace method described on page 10398, line 13 (i.e. <2.5%), so no need to discuss this further here.

This was done in the revised manuscript.

3. Page 10402, lines 20-24: Do the investigators have the C/N ratio of the surrounding peats? Depending on the degree of microbial processing, many peats typically have C/N ratios similar to that of plant material (e.g. 40-60), with higer C/N values more common in forested peatlands with a larger proportion of woody debris. It is therefore likely that the DOC & POC consist of a mixture of phytoplankton, terrestrial plant material AND decomposing peat C, not simply the first 2 constituents.

This is a valid point. Although we do not have C/N ratios for peat, we added the possibility of decomposing peat as a source of POM with reference to C/N ratios measured by Baum 2008 in Indonesian peat.

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4. Page 10402, line 25: Do any data exist on the 15N values of organic material? These may provide useful insights into the degree of N-limitation in the system.
d15N analysis was only performed on 2014 POM samples. Those samples were first analyzed for C/N contents and then for isotopes. However, the amount of each sample available for isotope analysis was insufficient for the determination of d15N with the exception of three samples. Of those three samples, only two were taken inside the national park and one was taken downstream of a waste water treatment facility outside the national park. Therefore, only two samples are actually representative of our study system. Their d15N values are 2.35 and 2.24 ‰ which is very slightly enriched if compared to peat d15N (Baum, 2008), suggesting that fractionation is small and that the system is N-limited. However, this data seems too scarce to support an actual statement about nutrient limitation, also seeing that the origin of POM cannot be assigned to only one source, which means that the d15N value is a mixed signal. Therefore, d15N is not so insightful, which is why we prefer to leave it out of the revised manuscript.

5. Page 10404, lines 15-22: With respect to CO2 fluxes, it is possible that some of this apparent "spatial" variability may also be reflective of temporal variability/antecedent conditions. For example, if there were sustained winds or large gusts prior to sampling, surface waters may have become depleted in CO2 due to enhanced outgassing driven by turbulent flow. In addition, spatial and temporal variability in conditions might synergistically interact. For instance, if certain stretches of river are more protected from the effects of wind, it is possible that they will (relative to more exposed reaches) show consistently higher dissolved CO2 concentrations and higher apparent diffusive fluxes, because there could be less turbulent fluxes from the water-atmosphere interface.
We added this point to the discussion.

6. Page 10405, lines 14-15: Consider slightly rephrasing the sentence "Enhanced CO2 is generally associated with oxygen depletion..." as this could be misinter-

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preted to mean that more anaerobic conditions reflect or are conducive towards greater organic matter decomposition. Revising this sentence could make the meaning clearer, e.g. "Enhanced CO_2 is generally associated with oxygen depletion, with lower oxygen levels reflecting high levels of organic matter decomposition and subsequent oxygen consumption by heterotrophs..."
We changed the sentence according to your suggestion.

7. Page 10405, lines 16-17: Part of the "natural variability" could also arise from the fact that this is an open system, and oxygen re-charge from the atmosphere could obscure/confound the effects of heterotrophic oxygen consumption. Without direct measurements of biological oxygen demand, it would be challenging to find very strong relationships between CO_2 and O_2.
Unfortunately, we do not have measurements of BOD. As we expected O_2 to be replaced approximately as quickly as CO_2 is released, we expected a somewhat stronger relationship between CO_2 and O_2. However, as the data shows, due to the fact that it is an open system, as you say, the relationship is not very strong. We added some additional discussion in the revised manuscript.

8. Page 10406: With respect to the 14C data, would it be possible for the authors to estimate or speculate as to what proportion of the DOC was arising from recent material and how much from older carbon? Do the authors have 14C estimates for the peat material and more recent plant compounds? The 14C data (potentially combined with 13C data) could assist in partitioning the decomposition sources into old versus recent material, depending on the precision of the 14C measurements.
Unfortunately, we do not have 14C data for peat, for leaves and neither do we have 13C data for DOC (only POC). However, Raymond et al. (2007) used an age attribution model in order to resolve the contributions of different time periods to the bulk DOC. If we use the age attribution model for the years after 1970 (as did Raymond et al. 2007), assuming that the peat column is intact and that the bulk DOC-age is

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indicative of post-bomb carbon, we find that 71 % is less than ten years old, and 91 % is less than 20 years old. This is in line with the average age obtained using the CaliBomb program. Note that this age attribution model has certain limitations, e.g., the 14CO2 curve used (Hua et al., supplemented with data from Levin et al.) extends only until 2012; and the assumption of a constant decay rate down the peat profile (Raymond's model 1) seems unwarranted. At the same time, this age attribution model does not change or improve our interpretation, which is, that DOC is mainly derived from recently fixed carbon. Given those limitations, we would prefer not to include the age attribution model in the revised manuscript.

9. Page 10406 – 10407, section 4.2: It would be useful, within the context of understanding the effects of land-use change, if the authors could draw some comparisons with fluvial C fluxes from managed/human-affected tropical peatlands. Simply speaking, are the fluxes from this near-pristine system on par, lower or greater than for human-affected systems?
There are some constraints to this comparison. First, since the TOC yield seems to depend on the peat coverage, we have to find a system with the same peat coverage (100%) for comparison. Such systems were studied by Moore et al. (2013), so they offer a point of comparison. However, the second challenge are the different characteristics of the peatlands, e.g., due to higher precipitation in Sarawak if compared to Central Kalimantan. Nevertheless, we compared our data to that of Moore et al. and discussed those constraints.

10. Page 10407, section 4.3: Two points; first, similar to point 9 above, would be comparisons of gas evasion from this system compared to managed systems (if these data exist). Second, could the authors elaborate on this concept of short residence time and CO2 concentration/gas evasion rate? From other studies, what would be considered moderate or long residence times? How would this difference influence CO2 fluxes and what type of mathematical relationship does water residence time have on gas evasion rates? E.g. is gas evasion rate linearly

related to water residence time?

First point: Unfortunately, this data does not exist. To our knowledge, there is no published estimate of CO2 evasion from a tropical peat-draining river, managed or unmanaged, other than ours.

Second point: We don't know of any publications that have established a relationship between in-stream water residence time and gas evasion, but it was mentioned before that it is an important factor (e.g., Cole et al. 1994, Battin et al., 2008, 2009; Moody et al. 2013), mainly for two reasons: Firstly, longer in-stream residence times facilitate equilibration between soil and water. Secondly, DOC decomposition occurs exponentially over time (e.g., Rixen et al., 2008), and therefore, decomposition rates are integrated over time. As a result, the relative amount (in %) of DOC decomposed in a peat-draining stream depends on in-stream residence times (Moody et al., 2013). Conclusively, in systems where in-stream DOC decomposition is a relevant source of CO2, in-stream residence time exerts a control on the buildup of CO2 in the stream (as suggested for lakes by Cole et al., 1994 and for a Hawaiian river by Paquay et al., 2007). When residence times are short, a relatively smaller fraction of the DOC will be degraded, and CO2 buildup is moderated. We added some of this additional explanation and the additional references in the revised manuscript.

11. Page 10408-10409: With respect to the uncertainty estimates, one thought I had is that it might be possible to show the range of estimates for the different fluxes in a table? For example, reporting the median, mean, range, minima and maxima for each of the fluxes? This might be a straightforward way of condensing this information.

We provided a summary table for the 2014/2015 TOC/CO2 data and the TOC and CO2 fluxes as suggested.

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